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AUTHOR Rostker, Bernard
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ABSTRACT

The paper provides the basic concepts and structure of an evaluative-management information system that meets the needs of vocational rehabilitation program and project managers and allows inter- and intra-program and project comparisons. Cost-benefit analysis has not (1) provided managers with relevant information upon which to base their decisions, (2) provided a systematic means for making tradeoffs, nor (3) helped managers develop consistent incentives and standards to measure internal project efficiency. Three major factors must be agreed upon before developing an evaluation system: quantifiable program objectives, benefit-cost framework, and a control group to be used as a standard for estimating gains in client income attributable to the program. Som problems to be considered in structuring mathematical models are estimating client income gain and using a national labor market survey as a statistical control. The mathematical relationship between sample survey mean, sample survey size, and the prediction of foregone income for single clients and aggregations of clients influences the prediction variance. The new design for a national longitudinal survey is a continuous work history survey based on calendar months which allows for complete recording of single employment, multiple employment, job search, training, and withdrawals from the labor force. (SAuthor/AG)

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The Rand Corporation
Santa Monica, California 90406

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AN EVALUATION-MANAGEMENT INFORMATION SYSTEM FOR VOCATIONAL REHABILITATION

By

Bernard Rostker*

*The author is a senior staff economist at The Rand Corporation. The views expressed in this paper are the author's own and do not necessarily represent those of The Rand Corporation, or its research sponsors. The author would like to thank David Greenberg, Gus Haggstrom, and C. Robert Roll for timely and perceptive comments on earlier drafts.

I. INTRODUCTION

One of the most important functions performed by government is the provision of manpower and vocational rehabilitative services.* Over the last 15 years these services have become increasingly large in scale and broad in scope. Moreover, the clients served and the techniques used have undergone continuous change. Because of these changes and those that will undoubtedly take place in the future, it is important that program and project managers are continuously able to measure the value of the various services provided. Such measures can become an important management tool and provide guidelines for decisions about how available funds should be allocated between services and whether the returns from a given service justify the resources expended on it.

Although considerable time and energy have been spent collecting statistics on current programs and in performing benefit-cost studies of special projects, the government does not have an effective evaluative program that accommodates the needs of either program or project managers. This paper lays out the basic concepts and structure of an evaluative-management information system that meets the needs of program and project managers and allows inter- and intra-program and project comparisons.

Plan of the Paper

This paper is divided into five major sections. The first section examines the general failure of benefit-cost analysis to provide project and program managers with relevant information upon which to base their decisions. It concludes that benefit-cost evaluation has been of little value because it has not provided a systematic means for making inter-program or even inter-project tradeoffs. Similarly, it has failed to help project managers develop consistent incentives and standards to measure internal project efficiency.

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*Federally supported vocational rehabilitation programs have been in existence for over 50 years. In the 1960s the traditional rehabilitation programs were expanded to include the mentally handicapped as well as the physically handicapped. In 1968 the socially and economically disadvantaged were also included. The main thing that distinguishes vocational rehabilitation programs from other manpower programs is their comprehensive approach to the problems of their client population. See: Marvin B. Sussman, ed., Sociology and Rehabilitation (Washington, D.C.: The American Sociological Association).

The second section examines three major factors which have precluded large-scale inter-program and project comparison and on which agreement must be reached before an evaluation system can be developed. The first factor is quantifiable program objectives. While not perfect, there seems to be some agreement that for employment related programs the best objective measure is improvements in the client's income stream after receiving program services. This will be the measure used in this paper.

The second factor which should be addressed before developing an evaluation system is the basic benefit-cost framework to be used. While a full development of such a framework is beyond the scope of this paper, it is clear that direct comparisons among projects which are evaluated with different benefit-cost methodologies are not possible.

The third factor addressed is the development of a control group which can be used as a standard to estimate the gain in client income attributable to the program. Several different control groups are examined, i.e., random, before-and-after, matched and sample survey-regression. For a variety of reasons the sample survey-regression technique is preferable when large-scale, continuing manpower programs are evaluated.

The third section examines the problems of estimating client income gain and the use of a national labor market survey as a statistical control. In practice, since follow-up information is collected at fixed intervals after closure, the problem is to see if income differentials exist over a fairly short period after completion of the program. Therefore, the length of the appropriate comparison period, and the specification of an appropriate regression equation to estimate (predict) the earnings foregone by program participants is critical.

Essential in such a procedure is a small prediction variance. If client's foregone income cannot be accurately predicted, then one has little information to evaluate the program. Consideration is therefore given in the fourth section to the relationship between sample survey mean, sample survey size, and the prediction of foregone income for a single client as well as an aggregation of clients.

The final section suggests an alternative design for national longitudinal survey. The development of an evaluation-management information system requires an appropriate labor market survey to be used as the basis for the

statistical control group. Unfortunately, existing longitudinal surveys do not adequately record labor market behavior. A new continuous work history survey design is developed, which is based on calendar months and allows for complete recording of single employment, multiple employment--both concurrent and sequential, job search, training and withdrawals from the labor force.

II. THE FAILURE OF BENEFIT-COST ANALYSIS

The decade of the 1960s witnessed a sharp increase in the use of benefit-cost analysis to examine questions of human capital formation and the evaluation of specific manpower programs.* Although these efforts were geared to "helping the decisionmaker," they had remarkably little effect on actual decisions. Originally, these studies were used to assess the "economic worth" of human capital investment programs. In fact, early studies (circa 1964) were read with great interest to determine the viability of the basic concept.** Unfortunately, aside from establishing the concept, these and subsequent efforts have been of little value to either

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*Benefit-cost analysis was initially used for evaluating alternative water resources projects. In the 1960s, with the growth of manpower programs under the Area Redevelopment Act and the Manpower Development and Training Act the techniques were extended to the area of human capital formation. This development was coincidental with that of the theory of human capital. Several classics in the development of benefit-cost analysis as a tool of applied economics are: Otto Eckstein, Water Resource Development (Cambridge: Harvard University Press, 1958); John V. Krutilla and Otto Eckstein, Multiple Purpose River Developments (Baltimore: Johns Hopkins Press, 1958); Charles J. Hitch and Roland McKean, The Economics of Defense in the Nuclear Age (Cambridge: Harvard University Press, 1963); Roland McKean, Efficiency in Government Through Systems Analysis (New York: John Wiley and Sons, Inc., 1958). The best review of benefit-cost analysis is: A. R. Prest and R. Turvey, "Cost-Benefit Analysis: A Survey," Economic Journal, 75 (December 1965): 683-735. Some of the major works in the development of human capital theory are: Gary Becker, Human Capital (New York: Columbia University Press, 1964); William Bowen, Economic Aspects of Education (Princeton: Industrial Research Section, Princeton University, 1964); Theodore W. Schultz, The Economic Value of Education (New York: Columbia University Press, 1963); Theodore W. Schultz, "Investment in Human Capital," American Economic Review, March 1961. Also see the October 1962 "Supplement" to the Journal of Political Economy. There have been numerous benefit-cost studies of specific manpower-vocational programs, many of which have been published in the Journal of Human Resources and the Industrial and Labor Relations Review. Also see: G. G. Somers and W. D. Wood, Cost-Benefit Analysis of Manpower Policies (Kingston, Ontario: Industrial Research Centre, Queen's University, 1969).

**See the early work of Gerald Somers and Ernst W. Stromsdorfer, "A Benefit-Cost Analysis of Manpower Retraining" paper prepared for the meetings of the American Economic Association and the Industrial Research Association, Chicago, December 28, 1964.

program managers or project managers, who have to make real resource decisions. For example, at the aggregate program level, Peter Barth argues that traditional

> ... evaluation of a single program is of limited usefulness typically, because it does not offer a set of feasible alternatives for making tradeoffs. An evaluation that is limited to the finding that a substantial net payoff exists for a specific program gives the planner no guide as to the other programs that might be cut back .... A related issue is the magnitude or degree to which good or bad programs are expanded or reduced. This question represents an enormous problem for the policymaker, yet evaluators have neglected it almost entirely.*

In addition, generalizations about large-scale programs that are based upon the outcome of traditional "small-scale" studies can be dangerous. Small-scale studies are seldom truly representative of the larger potential client population, nor are they indicative of the possible variations in project organization or diverse environmental settings.

If benefit-cost analysis has failed the program manager, it has been irrelevant and even burdensome at the project level. While evaluators like to talk about decisionmakers, few studies attempt to understand the actual decisions and options faced by project managers. As a result, studies are frequently ill conceived and poorly timed. The information they do provide is available ex post and does not affect real time resources decisions. A recent examination of these studies by the Urban Institute found that it was the basic

> ... design characteristics (of the traditional studies) which severely restrict their reliability and usefulness. (a) They have been one-shot, one-time efforts when we need continuous evaluation of program. (b) They have been carried out in terms of program categories and very weak on process data when we need analysis that gets at the underlying assumptions of these programs. (c) They have been small sample studies working with gross averages, when we need studies large enough to allow analysis of the wide natural variation we know exists in costs and performance among projects within programs.**

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*Peter S. Barth, "On Interprogram Manpower Studies," in Evaluating the Impact of Manpower Programs, Michael E. Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972, p. 4).

**John W. Scanlon, Joe N. Nay and Joseph S. Wholey, "An Evaluation System to Support a Decentralized, Comprehensive Manpower Program," in Evaluating the Impact of Manpower Programs, Michael E. Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972, p. 28).

The failure of benefit-cost analysis to deal effectively with project decisions is seen by the distinction that evaluators often make between process and outcome evaluation.* Process evaluation is defined as administrative monitoring and outcome evaluation as benefit-cost analysis. Although evaluators continue to talk of the net benefit derived from program participation, project managers continue to base resources decisions on such process indicators as case load, training class size and case closures. For example, in California an employment counselor was judged to be performing satisfactorily if he had 30 case closures a month. Similarly, Berkowitz and Anderson found that

> ... in the traditional [vocational] rehabilitation project, the concept of rehabilitated or not rehabilitated (employed--not employed) is strictly dichotomous. No consideration is usually given to the quality of the job which the person receives, and no comparison is made in the crude statistics between the job he held before entering the project and the one he is placed in after completing services. Thus a client whose training has benefited him to the extent that he has substantially improved his position in the labor market, is recorded in the same fashion as one who receives a menial job at perhaps less than the going rate, in the area.**

Traditional process indicators are meaningless in terms of the needs of the client population, and they establish perverse incentives and behavior inconsistent with program goals.

The consequence of measuring performance in terms of process indicators can be illustrated by considering two employment counselors. Assume one places 50 clients in menial jobs and another places half that number in substantial positions. Which has done the better job? Assume that the net benefit to the client population and society is greater in the second case. Although that should show up in the final benefit-cost analysis, the immediate

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*For example, see: Glen G. Cain and Robinson G. Hollister, "Evaluating Manpower Programs for the Disadvantaged," in Cost-Benefit Analysis of Manpower Policies, G. G. Somers and W. D. Wood, eds. (Kingston, Ontario: Industrial Research Centre, Queen's University, 1969, pp. 120-121).

**Monroe Berkowitz and Merilee Anderson, PADEC--An Evaluation of an Experimental Rehabilitation Project (New Brunswick: Bureau of Economic Research, Rutgers University, The State University of New Jersey, 1974, p. 31).

effect is to support behavior of the first counselor. If benefit-cost analysis established criteria against which the program and project are to be measured, those criteria should be reflected in the internal incentive structure of the project. It makes little sense to establish operating incentives that do not support program objectives or to evaluate a program in terms of one standard when those charged with carrying out the program march to a different drummer. Process evaluation should be consistent with output evaluation.

In sum, traditional benefit-cost evaluation has been of little value to the program manager it purports to serve, because it has not provided a systematic means for making inter-program or even inter-project tradeoffs. Similarly, it has failed to help project managers develop consistent incentives and standards to measure internal project efficiency.

III. PROGRAM AND PROJECT COMPARISONS

Before large scale inter-program and project comparisons are feasible agreement must be reached on several major issues.

1. Quantifiable program objectives
2. A consistent benefit-cost framework
3. A consistent, statistically acceptable and economical control group methodology.

Program Objectives

Any evaluation system must be based on an articulated set of goals that can be objectively related to acceptable output measures. Most programs operate under specific legislative mandates, but for a variety of reasons the specification of goals and the determination of proxies for output measures are not always successful. Development of consistent goals and proxies across programs with vastly different orientations may not be possible. It may never be possible to compare Head Start with a vocational rehabilitative program designed to service handicapped adults. However, within general program areas one should be able to specify consistent goals and proxies, and among projects that are part of a given program there should be no problem in establishing consistent measures of performance.*

In the area of manpower-vocational programs there is general agreement, based on specific legislative mandates, that the primary goal is improvement in employment performance of program participants. Evaluators of manpower-vocational programs appear to agree that for employment related programs the best objective measure of program and project performance is improvements in the client's income stream after receiving program services. Improvements in the income stream result from a reduction in unemployment, an increase in job stability, an increase in wages received, and allows comparison across programs and among projects that serve different client groups.** This will be the output measure used in this paper.

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*See: Cain and Hollister, op. cit., pp. 122-125.

**See Barth, op. cit., p. 6.

Even if there is agreement as to the primary goal of employment related programs, there is substantial disagreement on how to treat non-labor-market performance. This manifests itself as conflict between program managers and project operatives. The latter frequently argue that measures tied only to actual job performance fail to recognize subjective changes of "well-being" that are unrelated to labor market performance. While some quantification of such factors as changes in dependency status for the handicapped or improvements in antisocial criminal behavior may be possible, a full accounting is beyond present methodologies. Moreover, even if further work on social indicators eventually provides operational measures, the problem of finding a basis for making tradeoffs required for real decisions would remain.

The establishment of goals and proxies for use in evaluation involves more than an abstract exercise forced by evaluators. It is a prime factor affecting management control. If process and outcome evaluation should be consistent, then the translation of explicit goals into incentives and standards can greatly affect program operation. The case of the Job Agent program in California provides a clear example.

In 1968, the California legislature created the Department of Human Resources Development as a comprehensive manpower agency with the goal of improving services for the disadvantaged. To help implement these goals the legislature created a new class of employment counselor, known as job agents. They were modeled after the vocational rehabilitation counselor and were to "develop individualized placement plans leading to continued self-sufficient employment for eligible clients with the most difficult problems of unemployment." They were to develop "innovative, new and original ways of achieving continued employment for clients."* The Job Agent Program was unique in another way. The legislature mandated that:

> At such time as job performance standards have been developed and performance measurement is feasible, the director [of the Department of Human Resources Development] shall recommend to the State Personnel Board the establishment of a form of compensation for agents ... based primarily on the job agent's achievements in obtaining successful completion of training and employment goals by eligible persons.**

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*California State Personnel Board Examination Notice, "Job Agents." August 26, 1969.

**California Assembly Bill No. 1463, Article 3, Section 9701, p. 10.

In clear language the legislature defined the functions of job agents and directed that their performance be measured in terms of client employment.

The Director of the Department clearly understood that performance standards and measures would not only facilitate the legislative mandate but would extend management control. Without measurable standards there was no way to determine performance except by the subjective judgment of supervisors. By stipulating measurable standards, management at all levels had a consistent means of evaluating the performance of individual job agents. Furthermore, measurable standards could be used by management to make inferences as to what attributes successful job agents possess, which agents work best with which clients, and which social services are most useful in bringing about employment improvements. Similarly, measurable standards provided the job agent with a yardstick he could use to recognize what management expects of him and to determine how well he was responding to their expectations.

Management supported the establishment of goals and measurable standards as a means of extending their control over the job agent. For exactly the same reason the job agents themselves resisted these standards. Management and the State legislature appeared to view the goals of the program as quite specific: to place and to keep disadvantaged clients in meaningful employment. Many job agents interpreted the goals of the program much more broadly: to provide any service--not only those directly related to employment--agents felt would be beneficial to the disadvantaged community. They therefore resisted the development of specific goals or standards.*

Benefit-Cost Framework

Even when there is agreement that the primary goal of manpower-vocational programs is to improve the employment experience of clients, and that this can best be measured as a change in the client's income stream, direct comparisons among projects which are evaluated with different benefit-cost methodologies are not possible. Benefit-cost methodologies differ in the way they treat

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*See: David H. Greenberg, Bernard D. Rostker and Leonard V. Scifers, The California Job Agent Incentive Pay Plan: A Case Study for Manpower Management, The Rand Corporation, P-5068, August 1973.

secondary benefits, spillover effects, transfer payments, opportunity costs and foregone earnings; in the way they price and treat nonmarket behavior; and in their general viewpoint. Benefit-cost calculations can be made from the point of view of society, the individual client, and the government. While everyone might agree that the program produced a given change in the income of a client, the calculation of benefits and costs from the different viewpoints results in different measures of performance. For example, participation in a vocational rehabilitation program might result in a reduction in remedial services needed by a handicapped client. How should this reduction be counted for purposes of program evaluation? A case can be made that since the services provided represented real resources they should be considered a reduction in cost--a real benefit. However, such services are generally considered transfer payments, and there is a long tradition that from the view of society transfer payments should not be counted in calculating benefits and costs. The individual considers the reduction in services a loss, although it might signify an improvement in dependency which he would value as a nonquantifiable benefit. The government, which formerly provided the service, would surely consider a reduction in services demanded a benefit.

The development of an appropriate and consistent benefit-cost model is beyond the scope of this paper. However, problems caused by not having such a framework are not abstract. For example, in conducting a recent evaluation of the Perth Amboy Diagnostic and Employability Center by the Rutgers Bureau of Economic Research, the Bureau attempted to compare the Perth Amboy project with two similar projects undertaken in different parts of the country. They noted that the first comparison study was executed from the view of society (the Wisconsin Study), and the second was done from the view of the government (the Michigan Study). The Rutgers group counted earnings foregone during training as a cost. The Wisconsin study did not count foregone earnings at all. And the Michigan group, working from the viewpoint of the agency, deducted foregone earnings from the benefit stream. Michigan was the only group

to count the net decrease in economic dependency as a benefit, again because they took the perspective of the government.*

Control Group Methodology

Even when evaluators agree on quantifiable program objectives and an appropriate benefit-cost framework, they must find an acceptable way to estimate the gain in client income attributable to the program. In general, control groups are established to answer the question, "What would have been the client's income had he not participated in the program?" Choosing a control group that provides the best answer to this question is considered by most evaluators their most difficult methodological problems.

On purely statistical grounds, the most widely preferred control group is one formed by random assignment of potential program clients into two groups. The first, the treatment group, receives program services. The second is denied services, thereby providing the control. This method closely follows experimental procedures used in agricultural and medical research, as well as in psychological and sociological research. However, on a number of counts random assignment is inappropriate for the kind of continuous evaluation that is required by program and project managers.

Random assignment creates major practical, legal, and moral problems.** To a person who needs and desires program services but is randomly, and thus arbitrarily, selected for the control group, such a program is the ultimate hoax. In fact, since the potential client is recruited, screened, and tentatively accepted--at least until the random selection is made--the mere act of rejection implicit in being selected for the control group could sufficiently affect attitudes to negate the randomness of the control group. The ultimate absurdity of random selection is the contention that not only should control

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*Berkowitz and Anderson, op. cit., pp. 133-138. For a further discussion on this point also see: Einar Hardin, "Benefit-Cost Analysis of Occupational Training Programs: A Comparison of Recent Studies," in Cost-Benefit Analysis of Manpower Policies, G. G. Somers and W. D. Wood, eds. (Kingston, Ontario: Industrial Relations Centre, Queen's University, 1969, pp. 97-118).

**For a further discussion of these problems, see: David A. Miller, "On the Choice of Control Groups: Comment," and Abraham Stahler, "On the Choice of Control Group: Comment," both in Evaluating the Impact of Manpower Programs, Michael E. Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972).

assignees be prevented from receiving program services, but that in order to ensure the research design and "in order to create a sufficiently long-after treatment period of observation on both groups, he (the evaluator) must normally arrange to bar those randomly selected for no treatment from entering substitute manpower programs for a specific length of time."*

Random assignment is inappropriate on other grounds. In practical terms random assignment is best administered in a small-scale study, occurring in a specific place at a specific time. However, it is just this type of small-scale, one-shot study that has failed to provide the required information to either program managers or project managers. Random assignment may be useful in testing out initial concept, such as income maintenance, or in determining the general response to alternative health insurance plans, but it is not applicable to the evaluation of large-scale, continuing manpower programs.**

If random assignment is not feasible or even appropriate for the evaluation of continuing manpower programs, are there alternative methodologies that allow estimation of client income gains? The answer is a qualified yes. In general, "Statistical control in a nonrandom setting becomes adequate only when all important variables affecting the dependent variable are measured and are included in tables or equations in proper mathematical form."*** While alternative methodologies currently available may not

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*Einar Hardin, "On the Choice of Control Groups," in Evaluating the Impact of Manpower Programs, Michael Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972, p. 55). Hardin does note, "Little opposition is likely to arise (to random assignment) when the benefits to the individual are somewhat uncertain, when persons excluded from the program do not long remain barred, when random assignment is only among forms of treatment and treatment of some kind is offered everyone, or when there are more well-qualified applicants than program vacancies. In opposite circumstances, randomization may not be feasible." Unfortunately, in the evaluation of large-scale and continuing programs none of Professor Hardin's conditions are met.

**For a further discussion, see: Cain and Hollister, op. cit., pp. 126-128.

***Hardin, "On the Choice of Control Groups," op. cit., p. 54.

satisfy the statistical purist, they can help meet the "rules of evidence" and provide considerable help in evaluating manpower programs.*

One of the most widely used approaches to evaluate vocational rehabilitation programs is based on comparisons of client's income before and after participation in the program.** In past studies this technique was used because it was inexpensive--no independent control group was constructed--and the necessary data were readily available. Unfortunately, this technique is based on a set of unrealistic assumptions. For example, if a client was unemployed before entering the program the "before and after" technique assumes that he will remain unemployed; i.e., his future income stream, had he not participated in the program, is zero. This is most unlikely and provides an overestimate of program performance.

Two other nonrandom techniques provide a better basis for establishing the performance of a program of given project. They are the "matched control group" and the "sample survey control group" methods.*** To develop a matched control group the evaluator seeks a group of individuals having similar characteristics to those in the treatment or program group and compares the responses of the "treated" and the matched groups. In practice there are many sources from which evaluators can draw a matched control group. Several early studies of manpower training drew matched control groups from the files of the local offices of State Employment Service. Earl Main used a "snowball" method by having each program participant identify a friend, neighbor, or relative who was in a similar economic situation. In his review of past studies, Hardin characterizes matched control groups as coming from the potential treatment population, from program dropouts, from the same high school, or from a group of qualified interested non-enrollees.****

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* For a further discussion see: Cain and Hollister, op. cit., p. 120 and pp. 149-152.

** This approach was used in evaluating the Perth Amboy experimental rehabilitation project. See: Berkowitz and Anderson, op. cit., p. 121. In addition see: Ronald W. Conley, "A Benefit-Cost Analysis of the Vocational Rehabilitation Program," The Journal of Human Resources, IV, 2 (Spring 1969), pp. 226-252.

*** For a rigorous statistical treatment of these methods, see: Gus Haggstrom, A Comparison of Alternative Methods for Estimating Treatment Effects, The Rand Corporation, P-5067, August 1973.

**** See Hardin, "On the Choice of Control Groups, op. cit., pp. 45-53.

In the survey control group, the evaluator uses sample survey techniques to collect an extensive set of socioeconomic, personal, and environmental data from the population at large. The exact data collected include factors that directly affect employment and labor market performance and that should be held constant when the effects of the program are being determined. Since the members of the survey control group are not "identical" to the members of the treatment group, the differences between the two groups are statistically controlled, using analysis of covariance or other regression techniques.

Neither the matched control group nor the survey control group techniques can guarantee unbiased estimates of program performance. Neither technique can guarantee that the treatment group does not differ from the control group in some attribute not accounted for in the set of characteristics used to make the match or explicitly entered into the regression equation. However, the potential bias can be reduced if a set of explanatory variables is chosen that theory and past empirical studies have shown to be important determinants of labor market behavior. Although not ideal, the national control group may be superior to the matched control group in this regard because the evaluator can control for more characteristics in the regression equation than he is able to in making the match.*

The survey control group appears to be superior to the matched control group on several other grounds. First, by using survey data and the regression model of the labor market performance of control group members, the evaluator

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*As Herbert Parnes and John Shea noted, "The Longitudinal Surveys are admirably suited ... (as a survey control group) because they provide measures of a substantial number of characteristics that are related to labor market success and that are also likely to be related to the probability of an individual applying for and being accepted into a manpower program ...

"Moreover, by introducing such explanatory variables into a multivariate analysis of the experience of both the experimental and control group, one would be able to move farther toward isolating the independent effects of the manpower program than most studies that have not been able to use random control groups."

Herbert S. Parnes and John R. Shea, "The Use of National Longitudinal Survey Data," in Evaluating the Impact of Manpower Programs, Michael E. Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972, p. 194).

is able to estimate the treatment effect individually for each member of the treatment group--for each program participant. In theory this could also be done if the matched control group was made up of individual matched pairs, but in practice the quality of the match rarely justifies this procedure. (Later in this paper I develop a model of "client income gain" which uses this feature. The ability to estimate the treatment effect for each individual client and the ability to aggregate these results allows inter- and intra-project and program evaluation.)

Second, the survey control group is extremely economical and statistically efficient when used to evaluate large-scale, continuing manpower programs. The matched control group method requires each project to establish and maintain a "local" control group, which is costly. Furthermore, since the control group must be specific to a given project, control group observations from one project cannot be used in the analysis of another project. In practical terms, each locally matched control group would be small, especially compared to the size of local projects. However, the survey control group, because it uses regression techniques to control for differences between the control and treatment populations, can be used by all projects. The burden of establishing and maintaining the survey control group would be handled by a central, professional survey research center, rather than falling on local projects. Although a large sample survey would be costly, the costs would not be borne by any single project but would be spread and amortized over many projects or even programs. The single survey control group is also statistically efficient because all observations in the survey would enter into the regression analysis of labor market behavior, which is the basis for the statistical control. In effect, the size of the control group for each and every project would be increased, and, other things equal, it would increase the statistical reliability of the control group. (A full discussion of statistical validity and sample size is developed below.)

Third, the national survey and regression analysis can explicitly take into account factors affecting program participation. For example, the sample survey might include questions about a person's motivation concerning the program, or about the availability of similar programs which could reduce the selectivity bias caused by the nonrandomness of the control group.

Fourth, the national control group would establish a common standard against which all projects within a given program could be measured and against which alternative programs could be evaluated. Difficulties caused by the lack of a common control group can be seen by Stromsdorfer's experience in West Virginia. In his analysis of a single project he estimated that the gain in client income ranged from $511 to $1310, depending on the choice of control group. In another study Borus found a similar range ($424 to $1176), depending on the selection of control group.*

Fifth, the development of a national survey not only provides the basis for a common control group, it also provides an extremely useful data base for investigating labor market behavior. Such a survey would become a major source of empirical information that scholars could use to further our understanding of the employment performance, for example, of handicapped and disadvantaged persons. In turn, this added knowledge could be used by evaluators to develop improved statistical control groups.

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*See: Hardin, "On the Choice of Control Groups," op. cit., p. 47, and Hardin, "Benefit-Cost Analysis ...," op. cit., p. 105.

IV. THE MODEL

Given an appropriate national labor market survey, the expected behavior of program clients can be predicted from the actual behavior of statistically similar people in the survey population. This requires modeling the economic behavior of control group members and estimating the model using appropriate statistical techniques.

Measuring Income Differentials

In theory, the primary benefit derived from participating in the program is the measurable change in lifetime earnings, i.e.,

$$\beta = \sum_{t=1}^{d} \frac{Y_t - \hat{Y}_t}{(1-r)^t}$$

where*

Y_t = observed income in client in year t

$\hat{Y}_t$ = expected income of client in year t, had he not participated in the program, inferred from an analysis of the behavior of the survey population

d = expected years to live

In practice, evaluators never have observations of lifetime earnings for program participants. Typically, follow-up information is collected at fixed intervals; usually six, 12 or 18 months after closure.** The problem is to determine if there is an income differential over a fairly short period.

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*This equation, for ease of explanation, does not take into account the probability of death or other exogenous labor market factors. For a more complete specification see Bernard Rostker, The Economics of Manpower Retraining, Unpublished Master's Thesis, Syracuse University, January 1966, and Charles M. Grigg, Alphonse G. Holtmann, Patricia Y. Martin, Vocational Rehabilitation for the Disadvantaged (Lexington, Massachusetts; D. C. Heath and Company, 1970, p. 117).

**Borus and Buntz note some exceptions. See: Michael E. Borus and Charles G. Buntz, "Problems and Issues in the Evaluation of Manpower Programs," Industrial and Labor Relations Review, Vol. 25, No. 2, January 1972, p. 240. For a discussion of follow-up strategies, see: Berkowitz and Anderson, op. cit., pp. 55-65.

Evaluators often extrapolate this difference over the client's expected life. However, this assumes that the effects of the service as measured shortly after closure are constant and the differential does not decrease over time--a questionable assumption.*

In general, the effect of participation in a vocational program on the income of clients is the difference between predicted and observed income. Because programs attempt to improve the economic situation of clients by reducing current periods of unemployment, as well as increasing future wage levels and job stability, the appropriate comparison period begins when a client joins the program and ends after the post-closure follow-up period. Since the length of time a client spends in the program is variable, the comparison period for each client is also variable. Accordingly:

(1) $$\tau_t = Y_t - \hat{y}_t$$

where:

τ = income differential

Y_t = client's observed income in period t

$\hat{y}_t$ = client's foregone income--the income a person with the same set of characteristics can expect in period t, based upon the behavior of the national survey group

t = duration of program plus predetermined follow-up period

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*There is substantial evidence that earnings gain declines over time. See: G. G. Somers and G. H. McKechnie, "Vocational Retraining Programs for the Unemployed" in Industrial Relations Research Association, Proceedings of the Twentieth Winter Meetings, 1967, pp. 25-35; G. G. Somers and E. W. Stromsdorfer, "Benefit Cost Analysis of Manpower Retraining" in Industrial Relations Research Association, Proceedings of the Seventeenth Annual Meetings, 1965, pp. 172-185; M. E. Borus and E. C. Prescott, "The Effectiveness of MDTA Institutional Training Over Time and in Periods of High Unemployment." Paper presented to the Business and Economics Section of the American Statistical Association, December 1973.

By comparison, evaluative studies often compare client and control group income only for a fixed period after closure.* In such cases only benefits derived from improved wage rates and job stability are recorded. This fails to account for the possible positive effect the program had in reducing unemployment. Moreover, the program can also have a negative effect upon unemployment which must also be counted. Participation in the program can increase the time a person is without work by substituting the program for normal job search procedures and placing the client in an extended training program.** [Presumably, training is an investment, and earnings foregone during training will be more than made up by increases in post-training income.] Therefore, the appropriate comparison is from when a person enters the program, not from when he is placed.***

The estimate of foregone earnings for a client with a given set of characteristics is the predicted income $\hat{y}$, based upon the following regression using the appropriate national survey data.

$$(2) \qquad y = a + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5X_5 + b_6X_6$$

where:

y = future income over the next period

X_1 = vector of demograph characteristics

X_2 = vector of work history before the period

X_3 = vector of local labor market factor during the period

X_4 = current duration of unemployment, if employed $X_4 = 0$

---

*All before and after studies ignore the effects of the program on unemployment. However, studies which use more carefully constructed control groups also make this mistake. See Doris Hull, "WIN II Longitudinal Impact Evaluation: Comparison Group Issues and Recommended Methodology," Unpublished technical note from KETRON, INC. to Stan Markuson, Office of Evaluation, Manpower Administration, USDL, January 16, 1974, p. 8.

**Berkowitz and Anderson note, "Many clients were resistant to participating in work evaluation because of the financial pressures to find immediate employment. They evidenced similar reluctance when faced with the prospect of further training. In other words, long-range planning was clearly a luxury to them." Berkowitz and Anderson, op. cit., p. 52.

***For an alternative specification see: Bernard Rostker, An Econometric Model for the Evaluation of Manpower Programs, The Rand Corporation, P-4944-1, January 1974, pp. 3-5.

X_5 = vector of factors relating to program participation and the availability of similar services to the survey population

X_6 = vector of handicap factors, type and degree

Four features should be noted about the above equation. First, equation (2) is a reduced form equation. Since we are interested only in predictions of future income ($\hat{y}$), the problem of bias in individual coefficients can be ignored.*

Second, since the comparison between predicted and observed client income takes place from the point a client enters the program to some point in the future, the regression must also "look" towards the future. Demographic status (X_1), current duration of unemployment (X_4), service availability (X_5) and handicap status (X_6) all refer to a given point in time, presumably when the survey is taken. Work history (X_2) is retrospective and refers to the period before the survey. Future income refers to observed income between the initial and subsequent surveys. Labor market factors (X_3) refer to exogenous factors during the interval between surveys.

Third, since the comparison period is variable, equaling the time between a client joins the program and the post-closure follow-up, and since the surveys are conducted at fixed intervals, the prediction must be normalized as follows:

$$\hat{y}_t = \hat{y}_s \cdot \frac{t}{s}$$

where:

$\hat{y}_t$ = client's foregone income in period t

$\hat{y}_s$ = predicted income in period S--the income a person with the same set of characteristics can expect in period S, based upon the behavior of the national survey group

S = interval between surveys

t = comparison period--duration of program plus predetermined follow-up period

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*See: E. Malinvaud, Statistical Methods of Econometrics (Chicago: Rand McNally and Company, 1966), p. 545.

Fourth, $\hat{y}$ is predicted individually for each program participant and the estimates of the income differential, i.e., treatment effect, is available for each client. Since individual clients are the basic treatment unit, it is possible to aggregate estimates of the treatment effect by counselor, work center, treatment technique, project or program. Furthermore, the estimates of treatment effects can be used as the dependent variable in a multi-variate analysis of performance. This should provide project and program managers with information needed to evaluate services at any level of detail.

V. STATISTICAL VALIDITY

As argued above, the use of a survey control group requires that the expected behavior of program clients can be predicted from the actual behavior of statistically similar people in the survey population. Essential in such a procedure is a small prediction variance. If a client's foregone income cannot accurately be predicted, then one has little information to evaluate the program. The following section will indicate some of the general difficulties of prediction and suggest an already developed and tested model for choosing an experimental design for the sample survey that can minimize the variance in prediction subject to cost and other policy constraints.

Variance of a Single Prediction

Expression for the variance of a single prediction may be found in almost any standard test on statistics or econometrics.* For expository purposes, presented is the simplest of all cases--that of making a single prediction from a two-variable model.

Suppose the model is:**

$$Y = \alpha + \beta X + u$$

all the classical regression assumptions hold, and the parameters α and β are estimated in the least squares fashion. Then, the estimate of Y_0 for a given X_0 is,

$$\hat{Y}_0 = \hat{\alpha} + \hat{\beta} X_0$$

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*For example, see: J. Johnston, Econometric Methods (New York: McGraw-Hill Book Company, 1963, p. 36), and Arthur S. Goldberger, Econometric Theory (New York: John Wiley & Sons, Inc., 1966, pp. 169-170).

**Using Johnston's notation.

and the estimate of the variance of the prediction of Y, $\hat{Y}_0$, for a given value of X_0 is:

$$\text{VAR } (\hat{Y}_0) = \hat{\sigma}_u^2 \left[1 + \frac{1}{n} + \frac{(X_0 - \bar{X})^2}{\sum_{i=1}^{n} (X_i - \bar{X})^2} \right]$$

where $\hat{\sigma}_u^2$ is the estimated variance of the residuals, n is the sample size, X_i is the ith observation of the X's in the sample and $\bar{X}$ is the sample mean.

The above expression indicates that the variance of the prediction depends both on the sample size and the distance between the sample mean and the specific value of X_0 under consideration. In particular, for a given sample size, the further X_0 is from the sample mean, the larger the variance of the prediction.

The relationship between the variance of the prediction and the sample size is particularly important. As argued above, the survey control group may be relatively large. Since the regression model and the sample survey will be used as a basis for evaluating many projects, the cost of a large sample can be amortized over many users. Specifically, for a desired level of precision in prediction [VAR $(\hat{Y}_0) = \gamma$] the required sample size is

$$n = \frac{(1 + \Delta^2)\ \hat{\sigma}_u^2}{\gamma - \hat{\sigma}_u^2}$$

where: for any X_0 of interest Δ meets the condition

$$\bar{X} - \Delta\sigma_x < X_0 < \bar{X} + \Delta\sigma_x$$

The above indicates that the "goodness of fit" of the regression equation to the survey data presents a limit on the variance of the prediction. Clearly

$\gamma > \hat{\sigma}_u^2$ because one cannot have a smaller prediction variance than the residual variance of the regression.

The above also indicates that given an estimate of $\hat{\sigma}_u^2$ and a desired tolerance, there is always a sample size that will compensate for the difference between X_0 and the sample mean.

While the above is illustrative of errors in prediction and can be extended to a multiple regression model in an analogous fashion,* it does not capture the full extent of the variance when one aggregates predictions for project and program evaluation.

The Variance of Multiple Predictions

In the evaluation of large scale and continuous manpower programs one is generally less interested in a single prediction for a single client, than in an aggregation of many predictions and clients into a management unit. Such a unit might be an individual counselor, work center, treatment technique, project or program. In such a case, because a single regression is used to make all predictions, the individual predictions are not independent. Therefore, the variance of the aggregate prediction is not simply the linear combination of the variances of the individual predictions that make up the aggregate prediction,** but involves their covariances. For example, if there are K predictions generated from a set of K, X_0's, where

$$\hat{Y}_{0i} = \hat{\alpha} + \hat{\beta} X_{0i}$$

then

$$\text{VAR}\left(\sum_{i=1}^{K} \hat{Y}_{0i}\right) = \sum_{i=1}^{K} \text{VAR}\,(\hat{Y}_{0i}) + \sum_{i=1}^{K}\sum_{j=1}^{K} \text{COV}\left(\hat{Y}_{0i}, \hat{Y}_{0_j}\right) \text{ for } i \neq j$$

Since one cannot in general say whether the covariance terms are positive or negative,*** it is not possible to derive a simple expression, as done above,

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*See: Carl F. Christ, Econometric Models and Methods (New York: John Wiley & Sons, Inc., 1966, p. 551.

**The reason that the predictions are not independent is that all are generated from the parameters $\hat{\alpha}$ and $\hat{\beta}$. An expression for the variance-covariance matrix for a set of predictions from a single regression may be found in Christ, op. cit., p. 551.

***See Christ, op. cit., p. 338.

for the relationship between the sample size and the variance of the aggregate prediction.

A Further Note on Sample Design

The problem of choosing a sample design is very complex and entails more than just presetting tolerance levels, guesstimating residual variances and the signs of variance-covariance matrices. It involves careful consideration of the cost of the sample, and the relationship of the client population to the survey control group. Conlisk and Watts have developed a general way to construct an experimental design that is not only imaginative, but powerful and practical.* While a full exposition of their approach is beyond the scope of this paper, an operational version of their model can set up an experiment design (specify a sample) that will minimize the variance of any linear combination of predictions subject to a constraint on the costs of sampling. Further added weight can be placed in a subsample of particular interest. For example, one might be particularly interested in the behavior of the handicapped population and might thus be interested in minimizing the prediction variance for this subpopulation.

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*See: John Conlisk and Harold Watts, "A Model for Optimizing Experimental Designs for Estimating Response Surfaces," in Harold W. Watts, John Conlisk, D. Lee Bawden and Larry L. Orr, eds., Field Experimentation in Income Maintenance, Reprint 54, Institute for Research on Poverty, University of Wisconsin, 1970, pp. 150-56.

VI. A NATIONAL LONGITUDINAL SURVEY

The development of an evaluation-management information system for vocational rehabilitation requires an appropriate national labor market survey which measures economic performance over time, and which can be used as the basis for the construction of a statistical control group. Unfortunately, one of the major failings of present longitudinal surveys are the methods they employ to collect labor market and employment information. For example, the Income Dynamics Panel of the University of Michigan's Survey Research Center uses traditional "last week" and "gross period recall" questions.* While such questions are appropriate for the Census's Current Population Survey and are useful in measuring the amount of employment and unemployment, they do not measure the time-phasing of employment and unemployment. As a result, they provide little help in attempts to measure detailed labor market behavior.**

Recently, several efforts have been made to collect more detailed continuous work histories.*** However, present attempts, most notably the National Longitudinal Survey, have been poorly structured and have not adequately dealt with secondary (moonlighting) employment and job search. This section will present an alternative continuous work history design, which it is hoped will more accurately record the respondent's labor market experience.

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*An example of a "last week" question is, "What were you doing last week--working, looking for work, or something else?"

An example of a "gross period recall" question is, "How many weeks did you work in the last 12 months?"

**For an example of the problems caused by a lack of time-phasing of unemployment in the Income Dynamics Panel see: F. W. Blackwell, D. H. Greenberg, A. J. Lipson, B. D. Rostker, S. T. Wolfberg, Performance Rewards for Services to the Employable Poor: A Proposed Incentive Pay System for California Job Agents, R-1028-HRD, The Rand Corporation, June 1972, p. 55.

***For a review of recent efforts by the Bureau of the Census see: Marie G. Argana, "Methods of Collecting Work History Information," in Evaluating the Impact of Manpower Programs, Michael E. Borus, ed. (Lexington, Massachusetts: D. C. Heath and Company, 1972, pp. 71-78).

A Design for a Continuous Work History Survey

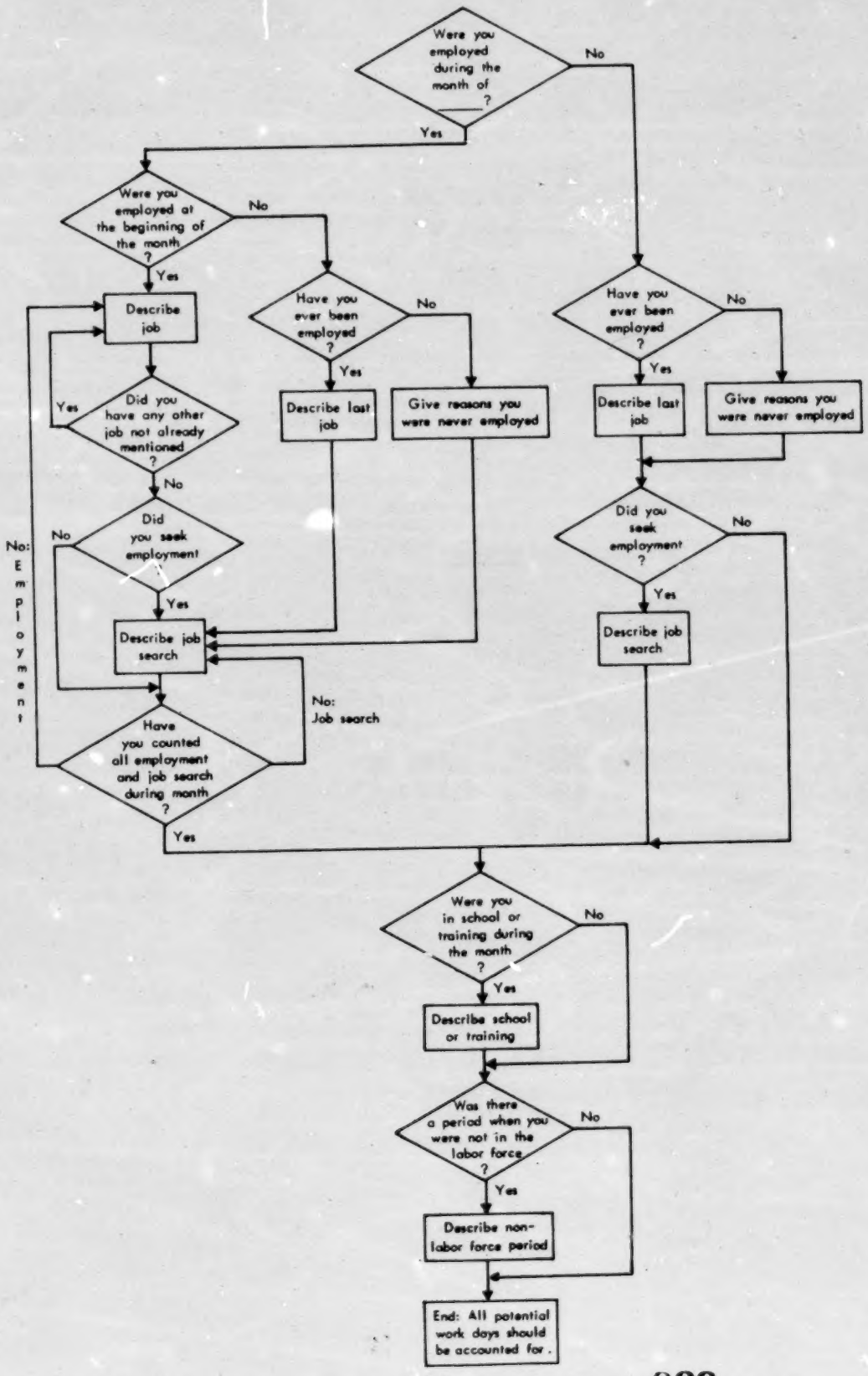
Most attempts to record continuous work histories have relied upon respondents to recall major periods of employment and unemployment. However, letting respondent define recall periods runs the risk that the respondent will omit relevant periods of employment and unemployment, both in the primary and secondary labor markets. Argana found that when using respondent defined periods "some jobs were omitted from the work history because the respondent forgot about them or because the questionnaire skip patterns did not allow for the job to be recorded."* The following survey scheme is based upon standard recall periods of single calendar months. Calendar months are a universal standard which relate to major events (holidays), and provide a common structure for comparison. In this scheme each month is treated as a discrete time period. Events that carry over from month to month are noted by determining if a particular behavior, i.e., a given job, occurred throughout the month or only at the beginning or end of the month.

For any given month the questioning starts by asking, "Were you employed during the month?" The exact flow and sequence of questions is described in Diagram I. The question flow is designed to account for each work day during the month and allows for single employment, multiple employment--both sequential and concurrent, job search, training and withdrawals from the labor force. There are two general types of questions--flow regulators, indicated by a diamond, and descriptive, indicated by a box. Flow regulators are designed to direct the respondent to a further descriptive section. An example of a flow regulator is, "Were you employed at the beginning of the month?" A descriptive section is designed to elicit information from the respondent. There are five major descriptive sections. They are: Last Job, Job Description, Job Search, Never Worked, Training, and Nonlabor Force Participation. The specific questions and responses for each of the major descriptive sections are presented as Figures I-V in the Appendix.

In the course of administering the questionnaire, and even within a given month, a respondent will be directed by a flow regulator to return to a major descriptive section several times. For example, in a given month, a respondent might be employed, fired, gone through a successful job search, then searched

*Argana, op. cit., p. 75.

Diagram I


Were you employed during the month of ____?
No
Yes
Were you employed at the beginning of the month?
No
Yes
Have you ever been employed?
No
Yes
Have you ever been employed?
No
Yes
Describe job
Did you have any other job not already mentioned?
Yes
No
Describe last job
Give reasons you were never employed
Describe last job
Give reasons you were never employed
Did you seek employment?
No
Yes
Did you seek employment?
No
Yes
No: Employment
Describe job search
Describe job search
No: Job search
Have you counted all employment and job search during month?
Yes
Were you in school or training during the month?
No
Yes
Describe school or training
Was there a period when you were not in the labor force?
No
Yes
Describe non-labor force period
End: All potential work days should be accounted for.

for a moonlighting job and found it. In such a case there would be three job descriptions (the first primary job, the second primary job, and the moonlighting job), and two job searches (the search for the second primary job and the search for the moonlighting job). Throughout the questioning, probing questions acting as flow regulators, direct the respondent to return to relevant sections until all work days are accounted for.

While the above continuous work history design has not been field tested, it is structured so that an accurate record of a respondent's labor market experience should be possible.

VI. SUMMARY AND CONCLUSION

During the past fifteen years there has been a marked increase in the government's provision of vocational rehabilitation and manpower services. The government has actively attempted to increase the employability of mentally, physically and culturally handicapped persons by providing a variety of rehabilitative programs. Concurrent with the development of these services has been the extension of benefit-cost analysis into areas of human capital formation and as a tool to evaluate specific vocational-manpower programs. While benefit-cost analysis has been helpful in assessing the viability of the basic concept of rehabilitation, it has proven of little value to program and project managers who, faced with limited budgets, have to make tradeoff decisions among programs and projects.

The failure of benefit-cost analysis to deal effectively with operational problems is seen by the distinction often made between process and outcome evaluation. Process evaluation is often defined as administrative monitoring. Outcome evaluation is defined as benefit-cost analysis. While evaluators continue to talk of the "net benefit" derived from an individual project, project managers continue to evaluate performance and make resource decisions on such process indicators as case load, training class size, and case closures. Such measures are not only meaningless in terms of the needs of the client population, they establish perverse incentives and behavior which is inconsistent with program goals. It makes little sense to establish operating incentives that are not supportive of program objectives, or to evaluate a program when those charged with carrying it out respond to different incentives.

There is a clear need for an evaluation-management information system which provides a systematic means for making inter-program and inter-project tradeoffs, and which establishes consistent incentives and standards to measure internal project efficiency. If such a consistent evaluative system is to be built there must be agreement on an articulated set of goals which can be objectively related to accepted output measures. In the area of vocational-manpower programs there appears to be general agreement that the primary goal is the improvement in the employment performance of program participants, and that this can best be measured as an improvement in the client's income stream after receiving program services.

While there is general agreement on the primary goal of vocational-manpower programs, there is little agreement on how to treat such things as dependence and nonlabor market performance. This disagreement often manifests itself in the different ways benefit-cost studies treat secondary benefits and spillover effects. While the full development of a benefit-cost methodology is beyond the scope of this paper, agreement must be reached before a consistent evaluative system can be developed.

A further problem which has prevented the establishment of a system to evaluate large-scale, continuing vocational-manpower programs is agreement on an appropriate control group and a methodology to answer the question, "What would have been the client's income had he not participated in the program?" While there are several control groups which could be employed, for a variety of reasons, the one best suited for the evaluation of large-scale, continuing manpower programs is one in which the client's foregone income is predicted, using a sample survey and regression analysis. In such a procedure a regression equation, whose dependent variable is income, is fitted to the sample survey data. The equation explicitly controls for--the independent variables are--demographic factors, work history, local labor market factors, duration of current unemployment, factors relating to program participation and the availability of similar services to the survey population, and factors relating to the type and degree of the handicap. The attributes of individual clients are substituted into the fitted regression equation to predict the income the clients would have had, had they not participated in the program. The difference between this projection and actual income observed after completing the program is the measure of income gain and program performance.

Critical to the above procedure is the variance of the predicted income. If income cannot be predicted within acceptable tolerances, then there is little reliable information on which to calculate income gain or to evaluate the program. In simple terms, the variance of the prediction is related to the goodness of fit of the original regression equation to the sample survey data, the distance in standard deviation units between the attributes of the client and the mean of the sample survey population, and the size of the original sample survey. If precision can be obtained in the prediction, i.e., the regression equation fits the sample survey reasonably well, the individual

client's characteristics are relatively close to the mean of the sample population and the original sample survey is relatively large, then this procedure will provide reliable information on which to evaluate the program.

The development of an evaluative-management information system requires an appropriate national labor-market survey which measures economic performance over time, and which can be used as the basis for the construction of a statistical control group. Unfortunately, a major failing of present longitudinal surveys is the method they employ to collect labor market and employment data. In its final section the paper outlines an alternative to existing continuous work histories. While the design has not been field tested, it is structured so that an accurate record of a respondent's labor-market experience should be possible.

In sum, the need for an evaluation-management information system to measure the effect of vocational-manpower programs is clear. Such a system must provide for inter-program and project tradeoffs, and must establish consistent incentives and standards. Finally, a national sample survey, with a well-structured continuous work history questionnaire and regression analysis, should provide an adequate control group for the evaluation of large-scale, continuous manpower programs.

APPENDIX

Figure I

LAST JOB

Questions	Responses
I. When did you first work on your main last job?	month, day, year
When did you leave your main last job? (If information was already collected as part of a previous month's response, do not repeat.)	month, day, year
II. Was it your practice to have more than one job at a time?	yes/no
III. Your last job was in what type of industry?	industrial code
what type of occupation?	occupation code
main--normal function?	write in
IV. When you worked:	
How many hours per day did you normally work at this job?	hours per day
How many days per week did you normally work at this job?	days per week
V. What time of day did you normally work?	varied/from (time) to (time)
VI. How much did you normally earn?	$_____per hr. or $_____day/week/ biweekly/monthly/ yearly/other
Why did you terminate from this job?	personal-family/ school/health/retirement/ seasonal job/slack work/ temporary job completed/ fired/quit/too far from home/poor working conditions/poor pay/to look for a better job

Figure II

JOB DESCRIPTION

Questions	Responses
I. Did you work the entire month on this job?	yes/no
If no, day (within month) job started?	day
If no, day job terminated.	day
II. Did you consider this:	
permanent or temporary?	permanent/temporary
main or secondary?	main/secondary
III. This job was in what type of industry?	industrial code
type of occupation?	occupation code
main (normal) function?	write in
IV. When you worked:	
How many hours a day did you normally work at this job?	hours per day
How many days per week did you normally work at this job?	days per week
During this month did you work less than the normal work period?	yes/no
If yes, why?	slack work/holiday/ labor dispute/bad weather/own illness/ illness in family/ vacation/did not want to work normal period
If yes,	
Did you average less hours per day?	yes/no
What was your average?	average hours
Did you average less days per week?	yes/no
What was your average?	average days
Did you work less weeks per month?	yes/no
How many?	weeks
During this month did you work more than the normal work period?	yes/no
If yes, why?	busy season/to fill in for another worker/ unusual increase in business/just wanted to work more
If yes,	
Did you average more hours per day?	yes/no
What was your average?	average hours
Did you average more days per week?	yes/no
What was your average?	average days

JOB DESCRIPTION (CON'T)

Questions	Responses
V. What time of day did you normally work?	Varied/from (time) to (time)
VI. How much did you normally earn?	$_____ per hour or $_____ day/week/biweekly/ monthly/yearly/other
VII. How much did you earn during this month?	$_____
Was this less than you normally earned from this job? If yes, why?	yes/no did not work the normal period/docked pay
Was this more than you normally earned from this job? If yes, why?	yes/no bonus/raise during month/ overtime
VIII. Did you terminate from this job during this month? If yes, why?	yes/no personal-family/school/ health/retirement/ seasonal job/slack work/ temporary job completed/ fired/quit, too far from home/poor working conditions/poor pay/better job offer/to look for a better job

Figure III

JOB SEARCH

Questions	Responses
I. Why were you looking for work?	supplement income/ substitute for my lost job/substitute for my present job/substitute for lost job of other family member
II. Were you searching for a job during the entire month?	yes/no
If no, day job search started?	day
If no, day job search ended?	day
III. When you started looking for work what type of job were you looking for?	
permanent or temporary	permanent/temporary
full-time or part-time	full/part
industry	industry code
occupation	occupation code
main function	write in
hours per day	hours
days per week	days
time of day	varied/start and stop time
IV. When you first started looking for work what was the minimum wage you would have accepted?	$_____per hour or $_____ day/week/biweekly/ monthly/yearly/other
V. Did the following provide help in trying to find work? Rate and Rank.	Rate as to: used very often used sometimes used frequently never used
public employment service	
friends and relatives	
want ads/radio/TV	
private employment agencies	
unions	
former employers	
other	Rate as to: quality of help 1 = most helpful
VI. How many job interviews with prospective employers did you have during the period you were looking for work?	number

JOB SEARCH (CON'T)

Questions	Responses
VII. Did you receive any firm job offers which you did not take?	yes/no
If yes, describe the best job offer you did not take.	
permanent or temporary?	permanent/temporary
full-time or part-time?	full/part
industry	industry code
occupation	occupation code
main function	main function
hours per day	hours
days per week	days
time of day	varied/start and stop time
earnings	$_____per hour or $_____ day/week/biweekly/monthly/ yearly/other
Why didn't you take this job?	too far from home/ poor working conditions/ poor pay/poor job stability
VIII. During the period did you change your mind about what minimum wage you would accept?	yes/no
What was your new minimum?	$_____per hour or $_____ day/week/biweekly/monthly/ yearly/other
When did you change your mind?	day
Why did you change your mind?	unemployed too long and needed work/lost too many jobs because of too high a min/better nonwage compensation
IX. How were you able to live--where did you get money to buy things when you were looking for work?	family/savings/welfare/ retirement income/other government payment/ charity/borrowed/ insurance/other
X. Did your search for a job end during this month?	found work/gave up and stopped looking/still looking
If found work, why did you take this job?	was what I was looking for/ not quite right but needed work

JOB SEARCH (CON'T)

Questions	Responses
If gave up and stopped looking, what were the reasons that you were unable to find work?	no job available/ employers think I am too young or too old/lack of skills/ lack of experience/ lack of education/ lack of references/ lack of tools/licenses, union membership/ language barrier/ physically handicapped/ police record/other
If gave up and stopped looking, why are you not looking now?	entered training school/ cannot arrange child care/ change in personnel situation

Figure IV

NEVER EMPLOYED

Questions	Responses
I. Have you ever looked for work?	yes/no
If yes, when did you first seek work?	month, year
If yes, when did you last seek work?	month, year
When you last looked for work, what was the reason that you were unable to find work?	no jobs available/ employers think I am too old or too young/ lack of skills/ lack of experience/ lack of education/ lack of references/ lack of tools/licenses/ union membership language barrier/ physically handicapped/ police record/other
II. Since you left high school have you spent a major portion of your time:	
in an institution?	yes/no
in a hospital?	yes/no
in the Armed Forces?	yes/no
ill at home?	yes/no
stayed at home?	yes/no
vocational school?	yes/no
III. How were you able to live--where did you get money to buy things?,	family/savings/welfare/ other government payment/ charity/borrowed/insurance/ other

Figure V

TRAINING

Questions	Responses
I. What type of training were you in?	regular high school/ college/vocational high school/vocational rehabilitation/business school/technical institute/OJT/work experience program
II. Is this a government sponsored training program?	yes/no
If yes, which one?	WIN/CEP/MDTA/Job Corps/ OIC/NYC/JOBS/other
III. Were you in school/training the entire month?	yes/no
If no, start and stop days.	days
If no, did you complete the program?	yes/no
If no, why did you not complete the program?	currently enrolled/found job/too much time involved/ lost interest/too difficult

Figure VI

NONLABOR FORCE PARTICIPATION

Questions	Responses
I. Were you out of the labor force the entire month?	yes/no
If no, start and stop days?	days
II. What were you doing when you were not in the labor force?	in institution/ in hospital/in Armed Forces/illness at home/ stayed at home/vacation/ school
III. Why were you not in the labor force?	illness/in institution/ in Armed Forces/ student/vacation/ did not want to work/ child care
IV. How were you able to live--where did you get money to buy things when you were not in the labor force?	family/savings/welfare/ retirement income/ other government payment/ charity/borrowed/insurance/ other